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Can Reminders of Rules Induce Compliance? Experimental Evidence from a Common Pool Resource Setting

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Abstract
This paper presents results from an RCT exploring whether a behavioural intervention can improve the conservation of a common pool resource. The literature on common pool resource management suggests that the existence of rules and sanctions is important to resource conservation. However, behavioural science suggests that individuals have finite cognitive capacity and may not be attentive to these rules and sanctions. This paper investigates the impact of an SMS message intervention designed to improve users' knowledge of and attentiveness to existing forest use rules. An RCT in Uganda explores the impact of these messages on forest use and compliance with the rules. This paper finds that SMS messages raise the perceived probability of sanctions for rule-breakers. However, SMS messages do not induce full compliance with forest use rules or systematically reduce forest use.

Keywords Common pool resources · Forest · Deforestation · SMS messages · Use rules · Sanctioning

JEL Classification Q2 · Q56 · O13

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1 Introduction

Deforestation leads to greenhouse emissions (IPCC 2019) and biodiversity loss (Betts et al. 2017; Barlow et al. 2016) and affects the livelihoods of rural communities, especially in developing countries. Policy interventions to solve this problem need to be tailored to local circumstances (Seymour and Harris 2019) and bear in mind that 28% of all forest across Africa, Asia and Latin America are managed by communities (Rights and Resources Initiative 2020). In light of the push towards decentralized forest management (Oldekop et al. 2019; Wright et al. 2016; Somanathan et al. 2009) it is of particular policy relevance to investigate how existing community-based and decentralized forest management can be improved. Behavioural interventions offer one way to make changes to conservation practices. Existing research indicates that behavioural interventions can be low-cost, while having a large impact on behaviours affecting environmental outcomes (see e.g. Carlsson and Johansson-Stenman 2012; Croson and Treich 2014; Brent et al. 2017; Schubert 2017 for reviews of related literature). In the context of community forestry—where a common pool resource is being managed—it is an open question whether behavioural interventions can improve forest conservation.

Early studies posited that common pool resources such as forests are doomed to be over-exploited. However, work by Ostrom (1990, 2009) suggests that if specific 'design conditions' are present, communities can sustainably manage common pool resources. Rules regarding resource use and graduated sanctions for rule-breakers feature prominently among these. Behavioural science meanwhile, suggests that individuals have finite cognitive capacity and may not be attentive to such rules, since benefits to resource conservation are diffuse, materializing only in the long term. Moreover, sanctions are only meted out with some probability.

This study combines insights from behavioural science and the literature on common pool resource management. It designs an intervention to increase users' knowledge of use rules and to increase attentiveness to these rules. The intervention consists of SMS reminders of forest use rules. We hypothesize that this intervention increases compliance with use rules, and decreases common pool resource use, through two potential channels. First, better knowledge of and attentiveness to use rules may increase users' scrutiny of others, and their willingness to sanction rule-breakers. A large body of literature has documented that sanctions can improve cooperation in common pool resource settings (e.g. Markussen et al. 2014; Rustagi et al. 2010; Ostrom et al. 1992; Visser and Burns 2015; Kosfeld and Rustagi 2015), particularly if the sanctioning regime is chosen endogenously (e.g. Sutter et al. 2010; Tyran and Feld 2006; Dal Bó et al. 2010). Alternatively, actual scrutiny and sanctioning may remain unchanged, but SMS recipients' *perceptions* of scrutiny and the likelihood of sanctioning may increase.

To investigate these hypotheses, we conduct a Randomized Controlled Trial (RCT) in 110 communities of common pool forest users in Uganda. Treatment households receive monthly SMS reminders of community-specific forest use rules. Data from household surveys allow us to assess the effect of the treatment on the knowledge of forest use rules, the probability of sanctions and compliance with rules. High-resolution satellite images as well as detailed on-the ground assessments of the forest are used to measure the effect of the SMS reminders on forest use.

Results indicate only weak evidence that SMS messages increase knowledge of forest use rules: forest users in communities subject to the SMS treatment have better self-reported—but not better actual—knowledge of forest use rules compared to other

communities in the study. We furthermore find that SMS messages increase the *perceived* probability of scrutiny and sanctions for rule-breakers, but do not affect *actual* scrutiny and sanctioning. SMS messages do not induce full compliance with forest use rules and they do not systematically reduce forest use. Although the estimates generally point towards a reduction in forest use, they are not always statistically significant.

This paper contributes to three strands of literature. First, this paper contributes to the growing literature on behavioural 'nudges' in environmental economics (see Carlsson and Johansson-Stenman 2012; Croson and Treich 2014; Brent et al. 2017; Schubert 2017, for reviews of this literature). The forest use SMS reminders studied in this paper are similar to nudges appealing to injunctive norms, [i.e. perceptions of behaviours that are typically approved of (Cialdini and Trost 1998; Cialdini 2003)]. Existing research mostly focuses on descriptive norms, informing individuals about their peers' behaviour, (Nolan et al. 2008; Goldstein et al. 2008; Ferraro and Miranda 2013; Demarque et al. 2015) or a combination of descriptive and injunctive norms (Schultz et al. 2007; Allcott 2011). SMS reminders in this study also appeal to social identity, in the same way that advertising campaigns appealing to a sense of community have been shown to significantly reduce littering in Texas (Grasmick et al. 1991). Since the effectiveness of nudges is often context-specific (Carlsson and Johansson-Stenman 2012; Schubert 2017; Carlsson et al. 2019), the present paper helps to understand how insights from existing studies in contexts such as water and energy consumption in developed countries (Allcott 2011; Allcott and Rogers 2014; Goldstein et al. 2008) translate into a developing country context, and to a different natural resource.

Second, this paper uses a field experiment to provide insights on common pool resource management, complementing existing observational studies [see Samii et al. (2014) and Bowler et al. (2010) for an overview] and lab-experiments. Observational studies suggest that endogenously emerging rules are one of the keys to success in community management of common pool resources. Whether outside initiatives can strengthen existing rules regimes is still an open research question. To answer this question, the present experiment exogenously varies knowledge of forest use rules and attentiveness to those rules.

Although the present experiment does not directly vary prevailing sanctions for rule-breaking (doing so would have serious ethical implications), it investigates whether SMS reminders affect scrutiny and sanctions within the community. This paper highlights a new mechanism through which sanctioning might induce cooperation: the *perceived* likelihood of scrutiny and sanctions. The effect of *actual* scrutiny and sanctions on contributions to the public good is well-documented, but behavioural science suggests that perceptions of scrutiny might also induce compliance with rules. Existing literature suggests that increasing a sense of being watched can increase compliance with rules, for example those regarding littering and contributions in a dictator game (Bateson et al. 2013; Nettle et al. 2013; Burnham and Hare 2007), and also that providing information on sanctions and increasing their perceived probability can raise compliance, for example with tax rules (Sanders et al. 2008; Iyer et al. 2010).

Finally, this paper complements the literature on the use of SMS messages to induce desired behaviour among recipients (e.g. Karlan et al. 2016; Schoar 2011; Dale and Strauss 2014). With the exception of Dale and Strauss (2014) this literature focuses on individually beneficial behaviour rather than contributions to public goods. Our paper presents the first application of SMS messaging to the management of common pool resources.

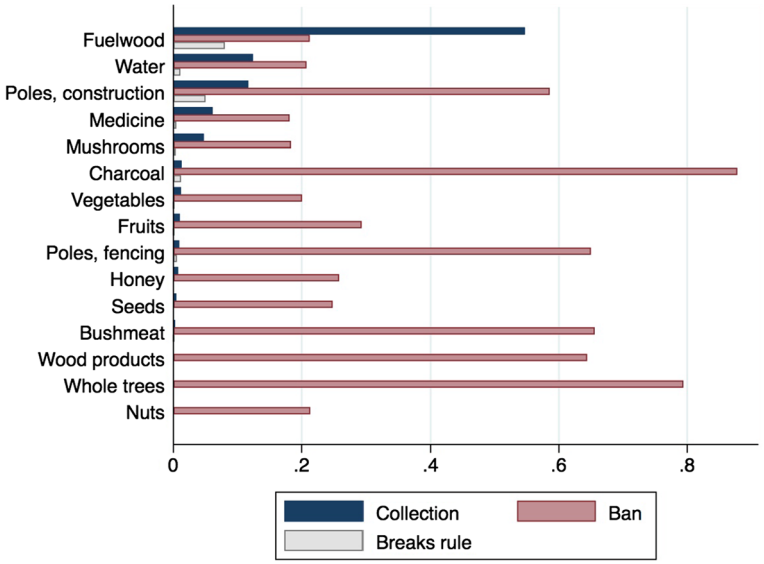

Fig. 1 Harvesting (bans) for forest products. *Note*: Collection: percentage of survey respondents collecting a forest product at baseline. Ban: Percentage of survey respondents living in a village where harvesting the respective forest product is completely banned in the community forest. Breaks rules: Percentage of survey respondents harvesting a forest product despite a harvesting ban

2 Setting: Community-Managed Forests in Uganda

This study is set in 110 villages in Central, West and South-West Uganda, all of which manage a common pool resource. Every village in the study has de jure management rights over a defined stretch of forest, which we will refer to as 'common pool forest'. Communities of forest users either have joint management rights with the government's National Forest Authority (NFA) over a section of a Central Forest Reserve, or sole management rights over a community forest.

Households in the study communities collect a range of products from the common pool forest, mostly for domestic use. The blue bars in Fig. 1 show the percentage of households collecting the respective forest products from the common pool forest at baseline. Fuelwood is the most important forest product: 54% of all households collect fuelwood from the common pool forest. Next, poles for construction are extracted by about 12% of all households. A smaller number of households fetch water (12%), collect medicinal plants (6%) and collect mushrooms (5%). Only 1% of the households report that they produce charcoal, and no household reports taking whole trees from the forest.

The prevailing forest management regime is chosen by (at least some subsection of) those living in the study villages. Participating in joint forest management with the NFA or registering a community forest requires that the community actively form a community-based organization to manage the forest, which either negotiates with the NFA or takes legal steps to register a particular stretch of forest as a community forest. The default management regime for a Central Forest Reserve is sole NFA management, while that for a community forest is private forest ownership.

Communities also have a say in the rules that govern the use of the common pool forest. In the case of community forests, these rules are set by a community-based organization managing the forest. In the case of Central Forest Reserves, rules are jointly set by a community-based organization and the NFA through a process of negotiation. The rules may vary from community to community, but there are similarities across communities. In most communities, the production of charcoal and timber cutting are completely banned.[1] Poles for fencing or construction can usually only be harvested with the permission of the community-level forest governance body, village elders or the NFA. The collection of fuelwood (and sometimes other forest products such as fruits or mushrooms) is often only allowed on two days of the week, and entry to the forest is usually banned on days which are not designated for the collection of forest products.

Formal sanctions upon breaking forest use rules exist in almost all villages in the study. These sanctions are enforced mostly by the NFA, but also by village-level authorities. Penalties range from fines and confiscation of forest products, to physical violence or imprisonment. The sanctions are imposed by the NFA in the majority of the villages. In some communities, sanctions are imposed by village authorities, community forest management bodies or the local government.

In addition to formal sanctions, informal sanctions—sanctions imposed by or triggered by fellow forest users—also exist. Such informal sanctions could range from scolding a fellow resource user for breaking the rules (more than 20% of surveyed village inhabitants report having done so in the last year), to reporting a fellow user to the formal authorities (13% of respondents report having done so).

At baseline, there is a high perceived likelihood that sanctions would be implemented among inhabitants of the village. Most surveyed village members (87%) state that it was likely or very likely that someone breaking forest use rules would receive a sanction and 6% report having received some sanction in the last year.

Despite existence of sanctions, forest use rules are frequently violated in study villages. Even though deforestation is forbidden, we found evidence of clear-cutting in more than one third of the villages. Average forest cover loss around households adjacent to the forest was estimated to be 1.2%, which is roughly as high as the national average (Global Forest Watch 2020). This is despite the fact that the forests in study villages are protected as Central Forest Reserves or community forests, and deforestation in these locations is forbidden. In 13% of villages, we found evidence of charcoal production, an activity similarly forbidden in virtually all villages. In the household survey, 15% of households report that they harvest forest products from the common pool forest in a way that would break at least one forest rule. The gray bars in Fig. 1 show the percentage of survey respondents reporting to harvest a forest product despite a harvesting ban.

Note that respondents were asked whether they harvested particular products, not to directly admit to breaking forest use rules. Respondents may indirectly admit to breaking forest use rules for a variety of reasons: respondents may be unaware of the rules, respondents may think the enumerator is unaware of the rules, the particular rule the respondent admits to breaking may not be strictly enforced, and the enumerator stressed that all responses were confidential.

[1] Figure 1 shows the percentage of households subject to harvesting bans for specific products.

3 Study Rationale and Hypotheses

Our main hypothesis is that SMS messages containing reminders of forest use rules increase compliance with forest use rules and decrease forest use. This main effect may be achieved if SMS reminders increase knowledge of and attentiveness to rules and therefore (the perceived likelihood of) scrutiny and sanctions for rule-breakers (auxiliary hypotheses). These hypotheses were specified prior to execution of this study's baseline data collection in a publicly registered pre-analysis plan (PAP).[2] Appendix section E lists any deviations from the PAP.

There is abundant evidence from several fields that SMS reminders can encourage desired behaviour among recipients. SMS reminders have been shown to encourage recipients to self-manage health conditions (Hall et al. 2015; Krishna and Boren 2008), save (Karlan et al. 2016), repay loans (Schoar 2011), register for benefits (Blanco and Vargas 2014), adopt productive agricultural practices (Larochelle et al. 2019) and vote (Dale and Strauss 2014).

SMS reminders may achieve this by increasing recipients' *knowledge* of what the desired behaviour is, or by *reminding* recipients with limited attention spans to engage in desired behaviour. The latter explanation has garnered more support in existing literature than the former. Karlan et al. (2016) and Larochelle et al. (2019) propose that individuals fail to engage in behaviour that is beneficial to them—meeting their savings goals and adopting better agricultural practices respectively—because the benefits of these actions are opaque and individuals with finite cognitive capacity are not attentive to them. Both sets of authors show that the recipients of SMS reminders are more likely to engage in beneficial behaviours. SMS interventions that mainly rely on providing information, such as information on eligibility for benefits to welfare recipients or market and weather information to farmers, have had small (Blanco and Vargas 2014) and no effects (Fafchamps and Minten 2012) respectively.

In the present study, individuals are reminded by monthly SMS messages of rules regarding forest use. In our setting, individuals' baseline knowledge of forest use rules is imperfect: on average, household survey respondents at baseline score their own knowledge of forest rules at less than 4 on a 5-point scale. Furthermore, as in the case of adopting better agricultural practices and saving, the benefits of forest conservation are opaque and will only materialize relatively far into the future. Therefore, we hypothesize that SMS reminders may increase both knowledge of forest use rules and attentiveness to forest use rules:

Hypothesis 1 (H1) SMS reminders of forest use rules increase knowledge of forest use rules.

Hypothesis 2 (H2) SMS reminders of forest use rules increase attentiveness to forest use rules.

Increased knowledge of or attentiveness to rules may lead recipients to *increase scrutiny of other forest users* and to sanction rule-breakers. Sanctions could consist of

[2] This PAP is available at: https://osf.io/dks3y. The PAP was updated prior to endline data collection and we are referring to the updated version of the PAP.

scolding others for breaking the rules or reporting others to the relevant authorities. A variety of studies suggest that individuals are indeed willing to incur costs to monitor and sanction rule-breakers at no direct individual benefit to themselves (Rustagi et al. 2010; Fehr and Gachter 2000; Fehr and Gächter 2002). Such behaviour may be restricted to individuals that Fischbacher et al. (2001) and Rustagi et al. (2010) classify as "conditional cooperators" (those who will contribute to a public good provided others also do so), and may be motivated by a sense of indignation or reciprocity (Carpenter and Matthews 2012).

Hypothesis 3 (H3) SMS reminders increase scrutiny of other forest users.

Hypothesis 4 (H4) SMS reminders increase willingness to sanction other forest users when they break forest use rules.

SMS reminders can also raise *perceived scrutiny by others*, and the perceived probability of formal and/or informal sanctions for rule-breakers, through three potential channels: (1) an increase in others' efforts to scrutinize or sanction; (2) higher attentiveness to sanctions; and (3) through a sense of being watched. It is worth noting that we can empirically distinguish between the first and latter two channels, but cannot distinguish among the latter two. The first channel follows logically from hypotheses H3 and H4. The second channel operates if recipients' greater attentiveness to forest use rules translates into greater attentiveness to instances of scrutiny or sanctions imposed on rule-breakers. According to the availability heuristic (Tversky and Kahneman 1973)—which posits that individuals judge the probability of an event by the ease with which a relevant instance comes to mind—this may result in a higher perceived probability of scrutiny or sanctions. Alternatively, the third channel suggests that receiving SMS reminders may instill a sense of being watched, raising the perceived probability of scrutiny or sanctions. In experimental settings, such a sense of being watched has been induced in absence of actual human interaction, for example by images of eyes (Bateson et al. 2013; Nettle et al. 2013; Burnham and Hare 2007). Note that for either the second or third channel to operate, *actual* scrutiny and the *actual* probability of sanctions need not change.

This leads us to the following hypotheses:

Hypothesis 5 (H5) SMS reminders increase perceived scrutiny by others.

Hypothesis 6 (H6) SMS reminders increase the perceived probability of sanctions upon breaking forest use rules.

Note that whether a household actually receives a sanction is a function of the probability of receiving a sanction upon breaking forest use rules, and the household's compliance with forest use rules.

Hypotheses 4 and 6 imply that the (perceived) costs of breaking forest use rules increase with SMS reminders, giving one of our main hypotheses:

Hypothesis 7 (H7) *SMS reminders increase compliance with forest use rules.*

If forest use in absence of SMS reminders exceeds levels allowed by forest use rules, and if H7 holds, SMS reminders decrease forest use. This gives our second main hypothesis:

Hypothesis 8 (H8) SMS reminders decrease forest use.

Some of the hypotheses above could also follow from a neo-classical model where SMS messages merely provide information, whereas other hypotheses rely on departures from this. In such a neo-classical model, individuals choose their level of forest use while being uncertain about the forest use rules (and whether their behaviour breaks them). A rational individual might choose to remain ignorant of forest use rules if acquiring information about rules is costly and the marginal costs of doing so outweigh the marginal benefits. The costs of obtaining information about forest use rules may consist of inquiring with or attending a meeting of a forest management authority. The benefits of having information about forest use rules could be to avoid any sanctions. SMS messages plausibly provide information about forest use rules at a low marginal cost, and therefore may improve knowledge of these rules, as per hypothesis H1. However, the impact on compliance with rules and forest use is indeterminate in a neo-classical model—contrary to hypothesis H7 and H8—and depends on an individual's prior beliefs about forest use rules. SMS reminders may make some individuals realize they have unwittingly been breaking forest use rules, decreasing their forest use, whereas others may realize that they have been mistaken in believing that they were breaking forest use rules, increasing their forest use.[3] These predictions resemble results obtained in experimental studies of the effect of information on willingness to pay for environmental conservation (see for example Munro and Hanley 2001; Needham et al. 2018).

Other remaining hypotheses also do not obviously follow from a neo-classical model. Such models do not predict that individuals would be willing to incur costs to monitor or punish others if there is no or only little direct benefit to themselves. Furthermore, receiving information about forest use rules would not obviously cause individuals to update their beliefs about the probability that sanctions are enforced (H5 and H6). However H5 and H6 could follow from a neo-classical model if individuals take the SMS message with information about forest use rules as a signal regarding the probability of enforcement. If individuals think that SMS reminders are sent by a third-party enforcing forest use rules, this could be plausible.

In addition to the hypotheses presented in this section, the PAP hypothesizes that SMS reminders of forest rules: (a) increase participation in forest governance bodies; and (b) increase satisfaction with forest use rules. The PAP furthermore states that the impact of SMS reminders on households' individual material welfare is indeterminate. In the short term, decreased forest use could decrease household income derived from the forest. In the longer term however, forest conservation might benefit the welfare of those households that rely on the forest for income. Results for these hypotheses can be found in Appendix section B. The treatment did not statistically significantly affect any of these outcomes.

[3] Furthermore, SMS messages might increase individuals' level of certainty about forest use rules. If individuals are risk-averse, they may increase forest use as a result.

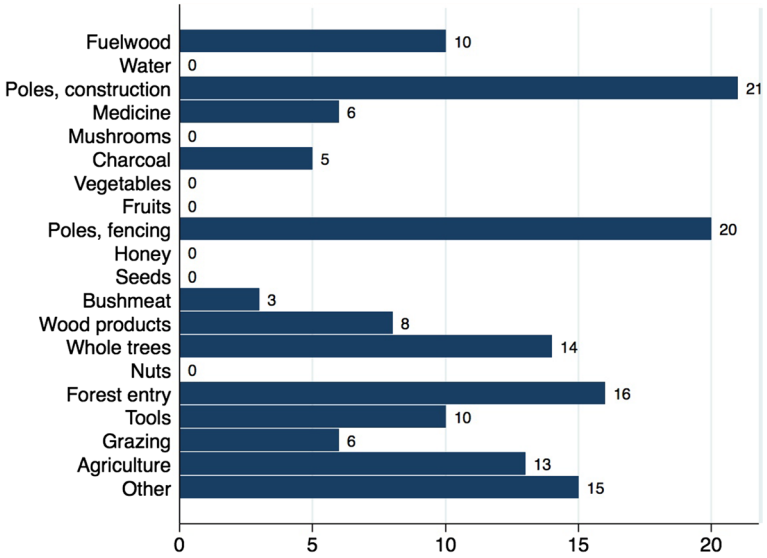

Fig. 2 Number of SMS reminders about different forest use rules. Note: The graph displays the number of SMS reminders about different forest use rules by village by month. E.g.: a message to 15 consenting households in one village in one month reminding households about rules regarding fuelwood collection is counted as 1. Some messages targeted multiple forms of forest use and are counted in several categories. Specifically, any rules relating to harvesting of timber are considered to affect harvesting of whole trees, wood products, poles for fencing and poles for construction. Rules regarding entry to the forest, tools, animal grazing and agricultural expansion into the forest are counted separately, not as affecting any individual forest products

4 Experimental Design

This study randomly assigns villages in Uganda with de jure management rights over a forest to receive SMS reminders of forest use rules and a second treatment. Out of the 110 villages in the study, 10 were assigned to receive SMS reminders. All of these 10 villages also received a second treatment, which encouraged community monitoring of the common pool forest. As explained in more detail in Sect. 4.2, villages which experimentally received only the monitoring treatment are the main comparison group.

4.1 The Treatment: SMS Reminders of Forest Use Rules

The SMS reminders aim to raise awareness and knowledge of forest use rules.[4] To that end, households receive monthly SMS reminders which clarify one of the community's forest use rules. Each month, a different rule is selected until all rules have been clarified, at which point rules are repeated. The text of the SMS message differs by village depending on the community's forest use rules. However, all households within a village that

[4] The treatment and treatment randomization as well as the empirical strategy follow a Pre-Analysis Plan (PAP), which was deposited in a repository prior to data collection and is available on https://osf.io/dks3y. Appendix section E lists any deviations from the PAP.

consented to receive SMS reminders received the same message, barring the recipient's name. An example of an SMS reminder of forest use rules is displayed below.

> Dear [name], please remember that *community members can only collect firewood on Wednesdays and Saturdays*. Thank you for obeying your community's rules.

The italics were added by the authors to highlight the component of the SMS message that varied across villages depending on the community's specific forest use rules.

Figure 2 breaks down the number of SMS reminders sent by type of forest use rule. Most SMS reminders were sent about poles for construction, poles for fencing and whole trees, which are among the products most commonly subject to a harvesting ban (see Fig. 1). No messages were sent about rules covering water, mushrooms, vegetables, fruits, honey, seeds or nuts, the harvest of which is not commonly banned. In addition to reminders of rules covering specific products, SMS reminders were sent about rules that prohibit or limit forest entry, effectively banning all harvesting, and the carrying of tools in the forest, likely designed to curb the most destructive harvesting practices. Lastly, several SMS messages were sent reminding recipients that using the forest to graze animals or grow crops, including burning down the forest to use it as agricultural land, is forbidden.

SMS reminders were sent to households in 10 treatment villages. Within these villages, SMS reminders were sent to all households that agreed to participate in the treatment. A total of 70 households across the 10 villages consented to receive SMS messages, with a varying number of SMS recipients across villages. Out of these, 40 households were covered by the endline household survey. Remaining households either failed to attend either community meeting or refused to consent to receiving the treatment.

4.2 Community Forest Monitoring as Second Treatment

The SMS reminders of forest use rules were part of a wider study investigating effective ways of reducing deforestation in common pool forests. All the villages receiving the SMS reminders treatment also received a second treatment, which will be referred to as the community monitoring treatment.

Under the community monitoring treatment, six community members systematically measure forest use and threats to the forest on a monthly basis. The monitors report information about collective forest use in village meetings and display their findings on a poster in a public place in the village. The community monitoring treatment is analysed in detail in a separate paper (Eisenbarth et al. 2020). For the purpose of this paper, the community monitoring treatment ensures that second-party monitoring is present in all villages receiving SMS reminders.

Throughout the paper, our main statistic of interest is the effect of the SMS reminders, where the comparison group is formed by villages who received the community monitoring treatment only. We will also report the joint effect of the SMS reminder and community monitoring treatment.

4.3 Study Population and Sample

The study population consists of households in 110 villages in Central, West, and South-West Uganda. To qualify for inclusion in the study, villages had to border a forest, and have de jure management rights over this forest. From the pool of potential villages, study villages were selected so as to avoid including neighbouring villages in the study. Hence, no two study villages with management rights over the same forest border each other.

Within each study village, 10 households were surveyed at baseline and endline. In communities receiving the SMS reminder treatment, all SMS recipients were sought for surveying at endline (some were not surveyed at baseline), though 30 of the 70 total could not be reached. If this led to fewer than 20 households being selected at endline in a SMS reminder community, additional households were surveyed until 20 endline surveys were reached. The sample for the household survey consisted of stratified randomly selected households, oversampling households bordering the common pool forest. Wherever possible, the same households were surveyed at baseline and endline. Where this was not possible, a replacement household was selected from the original sampling list.

Attrition from the baseline sample was 4.9% in the pure control group, 4.0% in the community monitoring only villages and 6.0% in the SMS reminder villages. Attrition is strongly balanced across treatment conditions ($p = 0.55$). Attrition is not significantly different in SMS reminder villages, when comparing these to community monitoring only ($p = 0.29$) or pure control ($p = 0.56$) villages.

4.4 Treatment Assignment and Randomization

Out of the 110 study villages, 50 villages were assigned to a pure control group (T0), 50 villages were assigned the community monitoring treatment only (T1), and 10 were assigned the community monitoring treatment and the SMS reminder treatment (T2).

We block-randomized villages by creating 40 pairs and 10 triplets, based on the Mahalanobis distance between villages. The Mahalanobis distance is a measure of how dissimilar two observations are in a multivariate space. This distance was calculated based on the baseline forest cover loss rate, baseline forest cover, and a forest identifier. The latter identifies the ten individual Central Forest Reserves included in the study, and considers all community forests as a single category.

4.5 Study Power

Power calculations were carried out during the design of the study and are captured in the PAP.

In addition, this paper re-calculates power based on realised sample sizes, as attrition in the study sample led to a smaller than expected sample. Importantly, these new calculations of power are not *post-hoc* power calculations: they do not use observed effect sizes or standard deviations. We do not use *post-hoc* power calculations as these suffer from several issues, as detailed in Zhang et al. (2019) and Lenth (2000). Instead, we maintain the same approach as in the PAP, and calculate the power of the study for a range of normalized effect sizes and intra-cluster correlations (ICC). Appendix section A gives more detail. In this paper, we do not speculate which results may be true nulls using observed coefficients for the same reasons. Rather, we briefly explore the power of our headline household level

findings here, and note that for some of our alternative specifications (most notably specifications which omit controls) sample size and hence power is higher.

At the household level, this study is well powered (greater than 80%) given an effect size of 0.5 standard deviations and a reasonably low ICC (less than 0.2). However, at lower effect sizes or a higher ICC, power is substantially lower. Previous SMS-based interventions in the field of health have seen a range of effect sizes (between 0.09 and 1.38), with the more effective campaigns achieving effects of more than 0.5 standard deviations (Hall et al. 2015), which this study would be powered to detect. This study also implements a repeat intervention (with each respondent being contacted multiple times over the course of the year), which can plausibly achieve greater effects than a one-shot intervention. Nevertheless, there may be some concern that null-effects instead represent small effects that the study is underpowered to detect. Therefore, Appendix section C presents analyses omitting control variables, which are powered to detect an effect size as small as 0.35 standard deviations given a low ICC, as per the PAP. No further statistically significant effects are found in these analyses compared to the main text.

5 Empirical Strategy

This paper estimates the effect of SMS reminders on knowledge of forest use rules, attentiveness to forest use rules, actual and perceived scrutiny and sanctions, compliance with forest use rules and forest use. This section describes how we measure these outcomes based on detailed data from household surveys and high-resolution satellite images. Moreover, it describes the estimating equations used to test the paper's hypotheses.

5.1 Outcome Variables

This paper combines data from household surveys, satellite data and on-the ground measurements of forest use to measure the effect of the SMS reminders on the outcomes specified in hypotheses H1–H8.

To avoid multiple comparisons, we test the effect of the treatment on a standardized summary index when several survey questions or outcome variables can be used to test a single hypothesis. Following Kling et al. (2007), these summary indices are simple averages across standardized z-scores. Z-scores are calculated subtracting the control group mean and dividing by the control group standard deviation. When only one variable is used to test a hypothesis, this variable is standardized as well unless otherwise specified. Where more than one outcome variable tests a single hypothesis, we will report p values obtained when implementing a multiple comparison adjustment (see Sect. 5.3).

5.1.1 Measuring Knowledge and Attentiveness Through Household Surveys (H1 and H2)

Household survey data are used to test the effect of the SMS reminder treatment on knowledge of forest use rules and attentiveness to forest use rules at the household level.

The *knowledge index* combines household survey questions on self-reported knowledge of forest use rules and measures of objective knowledge. To capture *self-reported knowledge of forest use rules*, we asked respondents how well informed they are about the rules regarding usage of the common pool forest. The response was measured on a 5-point

Likert scale, with higher values indicating that respondents judge themselves to be better informed. *Objective knowledge of forest use rules* is measured as an index based on four variables. We create three dummy variables if households correctly identify that there are rules limiting fuelwood collection, harvesting of poles for construction, and the production of charcoal, and an additional dummy variable if they know whether there are any rules limiting entry into the forest.

We use the normalized frequency of discussions about forest use rules as a proxy for *attentiveness* to forest use rules.[5] This measure is motivated by the idea that topics that individuals are attentive to are discussed more frequently. Attentiveness is difficult to measure and most papers analysing the effect of SMS reminders on behaviour assume that these achieve an effect through increasing attentiveness without measuring changes in this intermediate outcome.

5.1.2 Measuring Scrutiny and Sanctions (H3, H4, H5 and H6)

We distinguish between *scrutiny of other forest users* (H3), *sanctioning of others* (H4), and *scrutiny by others* (H5) and the *probability of sanctions by others* (H6). For all outcomes, higher values indicate more scrutiny or sanctioning. Measures of scrutiny and sanctions are derived from the household survey and components of the respective scrutiny and sanctioning indices are displayed in Table A.1 in the Appendix.

Scrutiny of other forest users takes higher values if households assisted in patrolling the forest in the previous year and if they think that they can easily detect a neighbour's violations of forest use rules.

Sanctioning of others is an index measuring households' self-reported willingness to sanction other forest users for hypothetical violations of forest use rules as well as actual sanctioning of others. A household's hypothetical willingness to sanction others is measured via a range of survey questions asking households whether they would scold neighbours or report them to the authorities in case they violate forest use rules. Moreover, the index includes questions measuring actual sanctioning of others in the previous year. Hypothetical sanctioning and actual sanctioning are conceptually different since actual sanctions only need to be meted out in response to observed violations. We will, thus, also present the treatment's effect on the two indices capturing hypothetical and actual sanctioning of others, in addition to the *Sanctioning of others* index, which captures both of these components.[6]

Scrutiny by others measures the perceived ease with which others would notice the respondent's violations of forest use rules.

Probability of sanctions by others measures both the *perceived* probability of sanctions by others (hypothetical sanctioning by others) as well as the extent to which the household actually received formal or informal sanctions for violations of forest use rules.

[5] This measure was pre-specified as part of a more broadly defined salience index. The pre-specified index makes use of an additional survey question "Would you agree with the following statement: It is important that people stick to the communal forest use rules even if they suffer a shock?". Since the latter question measures the extent to which norms are internalized rather than attentiveness, the index is not used here. However, the results are shown in Column 4 of Table B.2 in the Appendix.

[6] The distinction between hypothetical and actual sanctions was not pre-specified but is important on conceptual grounds.

5.1.3 Measuring Compliance and Forest Use (H7 and H8)

Non-compliance with forest use rules (H7). We define households as non-compliant with forest use rules if they report collecting a forest product even though collection of the product is completely banned. A non-compliance index captures non-compliance with harvesting bans for fruits, vegetables, mushrooms, nuts, honey, seeds, medicinal plants, bushmeat, fuelwood, poles for construction, poles for fencing, thatch for roof, wood processed into furniture or other wood products, whole trees, charcoal and water. For each of these products, we create a dummy variable that takes a value of 1 if the product is collected by a household in the common pool forest even though collection of this product in the common pool forest is banned. The dummy variables are aggregated into a non-compliance index which is the average over the z-scores for each of the variables.

A *second non-compliance index* captures compliance with those forest use rules that were specifically targeted by the SMS reminders.[7] This includes non-compliance with bans on the collection of fuelwood, poles, whole trees, and wood that can be processed, the collection of medicinal plants, bushmeat hunting and charcoal burning. Moreover, it includes household forest loss derived from satellite data. The latter variable measures non-compliance with rules forbidding agricultural land expansion and the cultivation of crops in the forest.

Neither of the two compliance indices capture compliance with harvesting limits or rules limiting the time at which a product can be harvested. However, an increase in compliance along those dimensions could be reflected by lower forest use. Therefore, we measure changes in forest use at the household level (hypothesis H8) through a *forest use index*. The forest use index is a continuous measure that can proxy for compliance with forest use rules, and capture increased compliance that falls short of full compliance. It reflects whether individuals harvest less of potentially banned products and move closer to compliance. Moreover, reduced forest use could reflect stronger compliance with upper limits for the harvest of forest products.

The *forest use index* index summarizes information on self-reported collection of all the above-mentioned forest products, self-reported clear-cutting of the forest and household level forest loss measured via satellite data. Self-reported collection of the different forest products is measured through count variables, which are often skewed towards zero with long right tails. Therefore, we transform these variables using the function $ln(1 + x)$ prior to the construction of the z-scores.[8]

The forest use index includes a measure of household level forest loss derived from satellite data. This captures the forest loss close to households' place of residence, and thus likely captures agricultural expansion into the forest. We use satellite data with a 10×10 m pixel resolution from the Sentinel-2 satellite. Presence or absence of tree cover in each pixel was determined using a Classification and Regression Tree (CART) classifier (Vaglio Laurin et al. 2016). For each forest-bordering household in either wave of the household survey, we construct a 200 m buffer around the point on the forest edge that is closest to the household. Within this buffer, we calculate the rate of deforestation as the number of pixels forested at baseline but not at endline relative to the number of pixels forested at baseline.

[7] This specification of the forest use index deviates from the pre-analysis plan, but we gratefully acknowledge a referee's suggestion to focus on behaviours that are target by the SMS reminders.

[8] This log-transformation was not specifically pre-specified but does not affect the paper's conclusions. See Appendix section E for details on deviations from the PAP related to the forest use index.

The deforestation rate was weighted to correct for differences in the amount of time that elapsed between baseline and endline satellite pictures.

Although our analysis mainly focuses on household-level measures of compliance with forest use rules and forest use, we also employ a *forest use index at the village level*. This was constructed using data from on-the-ground assessments and satellite data. Due to the small number of villages in the SMS reminder group, results at the village level should be taken with caution. However, using this data also has advantages, as it is not subject to reporting bias.

During on-the-ground assessments, the research team measured the number of *cut trees*, *animals grazing*, *charcoal kilns* and *cut branches* along two transects, or paths into the forest at baseline and endline.[9] Satellite data allow us to asses *forest loss* at the village level. We calculate the rate of forest cover loss for the area around the transects used for on-the-ground measurements and in the area within a 500 m buffer around the forest border between the two outermost surveyed households in a village.[10] Forest loss at the village level is calculated and weighted in the same way as forest loss at the household level.

5.2 Estimating Equation

The following pre-registered estimating equation is used to measure the effect of SMS reminders. Consider outcome variable Y, for household i[11] in village j which is assigned to randomization block m, at time t (measured in years). Time $t = 1$ indicates the post-treatment period. We estimate the following model using an ANCOVA specification:[12]

$$\begin{aligned} Y_{ijmt=1} = {} & \alpha_m + \beta_1 \text{Monitoring}_j + \beta_2 \text{Monitoring}_j * \text{Rules}_j \\ & + \beta_4 Y_{ijmt=0} + \delta X_{ijt=0} + \epsilon_{ijm} \end{aligned} \tag{1}$$

where $Monitoring_j$ is an indicator equalling one if the village is assigned to the community monitoring treatment and $Rules_j$ is an indicator equalling one if the village is assigned to the SMS reminder treatment. Since the villages assigned to the SMS reminder treatment are also assigned to the community monitoring treatment, β_2 measures the effect of the SMS reminders in villages assigned this treatment compared to villages receiving only the community monitoring treatment. The joint effect of the community monitoring treatment and the SMS reminder treatment is measured by the linear combination of $\beta_1 + \beta_2$. We report the linear combination of $\beta_1 + \beta_2$ and the corresponding p values at the bottom of each results table.

The regressors in Eq. 1 include a vector of randomization-block-fixed effects α_m, a set of control variables $X_{ijt=0}$ and the baseline level of the outcome variable $Y_{ijmt=0}$. Several

[9] Those transects were also monitored every month by the community monitors as part of the community monitoring treatment.

[10] Unlike pre-specified in the PAP, we did not record the entire village-forest border using on-the-ground measurements as this proved prohibitively time-consuming. Therefore, we provide two forest cover loss rates: (1) the forest cover loss rate in an area around the transects used for on-the-ground measurements, and; (2) the forest cover loss rate in a 500 m buffer along the entire village-forest border, where this border is imputed using satellite imagery.

[11] Note that for analyses at the village level, subscript i is redundant.

[12] We use ANCOVA specifications as they are weakly higher powered than difference in difference specifications, with this difference increasing as autocorrelation of the outcome variables decreases (McKenzie 2012).

village and household level survey questions were only asked at endline or had a poor response rate at baseline. In that case, $Y_{ijmt=0}$ is not included as a covariate.

For analyses at the household level, the vector of control variables, $X_{ijt=0}$ includes the gender, age and education of the respondent, the percentage of people among survey respondents that are of the respondent's tribe, whether the respondent is native to the village, the number of adult females in the household, whether the household borders the forest, whether the household is a member of the community-based organization managing the common pool forest, the age of this organization, whether the household owns a business, whether the household owns a panga (a tool used to cut branches from trees), whether the household owns a chainsaw, whether the household built a new building in the last year, the amount of land managed by the household, the size of the village, the travel time to the nearest financial institution, the travel time to nearest market for fuelwood, charcoal, poles and whole trees and the distance to Kampala. For analyses at the village level, this vectors includes village size, age of the community-based organization managing the common pool forest, an ethnic fractionalization index, the share of respondents born in the village, travel time to nearest market for fuelwood, charcoal, poles and whole trees, an average village asset index, and the percentage of the respondents in the village who own a business. The covariates were chosen either based on their high predictive power in Lasso regressions of potential baseline covariates on baseline outcomes, or because the covariate was unbalanced between treatment and control group at the 10% level.[13]

The inclusion of baseline control variables *de facto* excludes households only surveyed at endline from the sample. Appendix section C therefore presents results from specification 1 omitting all control variables. Although not pre-specified, this specification has the advantage of capturing more households in SMS reminder villages, as the sample of households in these villages was doubled at endline. However, omitting control variables does make estimates noisier.

Specification 1 constitutes an Intent To Treat estimator, as all households in SMS reminder treatment villages are regarded as treatment households, regardless of whether they consented to receive the treatment. Appendix section D gives results differentiating the effect of the SMS reminder treatment between inhabitants of treatment villages that received SMS messages and those who did not. As households within treatment villages could self-select into the treatment, the results presented in Appendix section D cannot be considered causal.

5.3 Multiple Comparison Adjustments When Looking at Index Components

To avoid multiple comparisons, we test the effect of the treatment on a single standardized summary index per pre-specified hypothesis. In some instances, we also look at components of the index. In this case, we conduct multiple comparison adjustments for the components of the index following the pre-specified method proposed by Benjamini and Hochberg (1995).

[13] Household cash income was also unbalanced between treatment conditions at baseline, but this variable has too many missing observations to be included as a control variable.

Table 1 Summary statistics and balance tests

Variable	(1) Mon, t = 0	(2) Rules, t = 0	(3) Mon, t = 1	(4) Rules, t = 1	(5) Diff. t = 0	(6) Diff. t = 1
Knowledge and attentiveness						
Knowledge			0.079	0.090		0.011
			(0.569)	(0.495)		(0.046)
Attentiveness			0.036	− 0.008		− 0.044
			(0.998)	(0.984)		(0.083)
Scrutiny and sanctioning						
Scrutiny of others			− 0.008	− 0.082		− 0.073
			(0.859)	(0.807)		(0.071)
Sanctioning of others			− 0.010	− 0.046		− 0.036
			(0.730)	(0.699)		(0.060)
Scrutiny by others			0.076	0.034		− 0.042
			(0.985)	(0.973)		(0.083)
Sanctioning by others			− 0.028	− 0.041		− 0.013
			(0.623)	(0.612)		(0.052)
Non-compliance and forest use						
Non-compliance	0.009	− 0.005	0.033	0.048	0.003	0.015
	(0.283)	(0.189)	(0.321)	(0.273)	(0.267)	(0.026)
Non-compliance 2	− 0.026	0.032	0.053	0.075	− 0.006	0.022
	(0.319)	(0.588)	(0.512)	(0.491)	(0.398)	(0.042)
Forest use	− 0.025	0.017	0.011	− 0.063	− 0.008	− 0.074**
	(0.319)	(0.592)	(0.413)	(0.334)	(0.384)	(0.032)
Forest use (village)	0.389	0.063	0.091	0.010	− 0.326	− 0.081
	(1.015)	(0.378)	(0.543)	(0.754)	(0.327)	(0.201)

The table reports average outcomes for households receiving only the monitoring treatment (Mon) and villages receiving both the community monitorinig and rules SMS reminder treatment (Rules) at baseline (t = 0) and at endline (t = 1). Columns (5) and (6) report differences in means at baseline and endline, respectively. The values in parentheses show standard deviations for the means (Column 1-4) and standard errors for differences (Columns 5–6). $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

5.4 Summary Statistics and Balance Test

Table 1 displays summary statistics for the main outcome variables. It also investigates whether outcome variables at baseline are balanced between those villages receiving the community monitoring treatment only, and villages receiving the community monitoring and SMS reminder treatments. When no summary statistics are given for the baseline, variables are only available at endline.

Outcome variables are balanced between both treatment conditions. There is no statistically significant difference between households in community monitoring only villages and households in SMS reminder villages in terms of baseline non-compliance with forest use rules or baseline forest use.

By a simple comparison of means, we observe some differences between villages receiving the SMS reminder treatment and other villages at endline. Household level forest use is significantly lower at endline in villages receiving the SMS reminder treatment.

Table 2 Knowledge of and attentiveness to forest use rules

	(1) Knowledge	(2) Self-reported	(3) Objective	(4) Attentiveness
Monitoring	0.124*	0.142**	0.138	0.143**
	(0.067)	(0.065)	(0.090)	(0.067)
Monitoring × Rules	0.221	0.267*	0.225	− 0.124
	(0.147)	(0.144)	(0.192)	(0.152)
$\beta_1 + \beta_2$	0.345	0.409	0.363	0.019
$\beta_1 + \beta_2$ p value	0.024	0.007	0.070	0.897
Control mean	0.002	0.101	− 0.009	−0.019
Lag dep. var.	No	Yes	No	No
Controls	Yes	Yes	Yes	Yes
Observations	915	570	915	916

Standard errors (clustered at the village level) in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

6 Results

Results give some evidence that SMS reminders of forest use rules increase knowledge of forest use rules, but not necessarily attentiveness to these rules. We do not find evidence that the treatment increases scrutiny or sanctioning of other forest users when they break forest use rules. However, the treatment is associated with a higher perceived likelihood of sanctions upon rule-breaking. The treatment does not raise compliance with forest use rules and does not systematically reduce household forest use.

6.1 Effects on Knowledge of and Attentiveness to Forest Use Rules

The combination of SMS reminders and community monitoring improved knowledge of forest use rules compared to the control group. The linear combination of β_1 and β_2 and the associated p value at the bottom of Table 2 reveal a statistically significant increase in knowledge by 0.35 standard deviations compared to the control group. Monitoring led to a statically significant increase in the knowledge of rules by 0.12 standard deviations and the SMS reminders raised knowledge by an additional 0.22 standard deviations. However, the coefficient estimate for the SMS treatment dummy variable is not significant at conventional levels.

A more detailed analysis reveals an increase in self-reported knowledge by 0.27 standard deviations compared to households receiving the community monitoring treatment and by 0.4 standard deviations compared to the control group. However, only the latter of those two effects is statistically significant once we have adjusted the p values for multiple hypotheses testing. Objective knowledge increased significantly compared to the control group.

Even though the literature suggests that SMS reminders should increase attentiveness to forest use rules, there is limited evidence of this effect in the context of our study. We use the frequency of discussions as a measure of attention to forest use rules and find that the SMS reminder treatment did not raise discussions about forest use rules (see Column 1 of

Table 3 Scrutiny and sanctioning of others

	(1) Scrutiny of others	(2) Sanctioning others	(3) Hypothetical	(4) Actual
Monitoring	0.040	0.027	0.016	0.046
	(0.046)	(0.042)	(0.049)	(0.048)
Monitoring × Rules	− 0.131	− 0.023	− 0.054	0.043
	(0.116)	(0.095)	(0.116)	(0.110)
$\beta_1 + \beta_2$	− 0.091	0.004	− 0.038	0.089
$\beta_1 + \beta_2$ p value	0.410	0.964	0.731	0.429
Control mean	− 0.034	− 0.017	− 0.022	− 0.005
Lag dep. var.	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Observations	914	916	915	916

Standard errors (clustered at the village level) in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4 Scrutiny and sanctioning by others

	(1) Scrutiny by others	(2) Sanctioning by others	(3) Hypothetical	(4) Actual
Monitoring	0.112	0.004	− 0.077	0.042
	(0.069)	(0.039)	(0.059)	(0.048)
Monitoring × Rules	− 0.159	0.149**	0.393***	0.033
	(0.151)	(0.072)	(0.132)	(0.076)
$\beta_1 + \beta_2$	− 0.048	0.153	0.316	0.075
$\beta_1 + \beta_2$ p value	0.732	0.040	0.022	0.374
Control mean	− 0.002	− 0.010	4.401	− 0.020
Lag dep. var.	No	No	Yes	No
Controls	Yes	Yes	Yes	Yes
Observations	907	916	876	916

Standard errors (clustered at the village level) in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2). However, we cannot rule out an increase in attention to forest use rules since the frequency of discussions is only an incomplete proxy of attentiveness. Higher attentiveness to forest use rules may be manifested through other channels than discussions.

Furthermore, it is worth recalling that all villages receiving the SMS reminder treatment also received a community monitoring treatment. As part of the latter treatment, meetings were organized for village inhabitants to discuss forest use. Although the protocol for these meetings did not include a discussion of forest use rules, results in Column 1 of Table 2 suggest that the community monitoring treatment was associated with an increase in such discussions. As such, the community monitoring treatment may already have pushed

Table 5 H4: Compliance and forest use

	(1) Non-compliance	(2) Non-compliance 2	(3) Forest use
Monitoring	0.015	0.017	0.026
	(0.020)	(0.036)	(0.022)
Monitoring × Rules	0.074*	0.081	− 0.065
	(0.040)	(0.081)	(0.064)
$\beta_1 + \beta_2$	0.089	0.098	− 0.038
$\beta_1 + \beta_2$ p value	0.027	0.208	0.554
Control mean	0.001	0.009	0.011
Lag dep. var.	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	910	960	963

Standard errors (clustered at the village level) in parentheses

$^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

treatment villages toward some upper bound of discussions about forest use rules, which may explain why the rules treatment did not have an additional impact on discussions.

6.2 Effects on Scrutiny and Sanctioning of Others

SMS reminders do not seem to raise scrutiny of other forest users. To the contrary, Column 1 of Table 3 shows that scrutiny of other forest users declines by 0.13 standard deviation in SMS reminder villages as a result of the treatment. However, the effect is not statistically significant.

Moreover, the SMS reminders of forest use rules did not have a significant effect on sanctioning of others, as demonstrated in Column 2 of Table 3. Looking at the components of the respective index shows that neither the hypothetical willingness to sanction others (telling a neighbour off for breaking forest use rules, or reporting them to the authorities for hypothetical violations of forest use rules) nor actual sanctions towards others increase as a result of the treatment (see Columns 3 and 4 of Table 3).

6.3 Perceived Scrutiny and Sanctions by Others Increase

Even though the perceived scrutiny by others did not increase in SMS reminder villages (see Column 1 of Table 4), the (perceived) probability of sanctions for rules violations increased as a result of the SMS reminder treatment. The latter finding is in line with hypothesis H4. Column 2 of Table 4 shows that SMS reminders raise sanctioning by others by 0.15 standard deviations compared to households in both monitoring treatment villages or pure control villages.

This result is driven by perceptions about the probability of sanctions rather than actual sanctions, as demonstrated by an analysis looking at different components of the sanctioning index. To look at perceptions, we use the standardized responses to the following household survey question as an outcome variable: “If a household in this

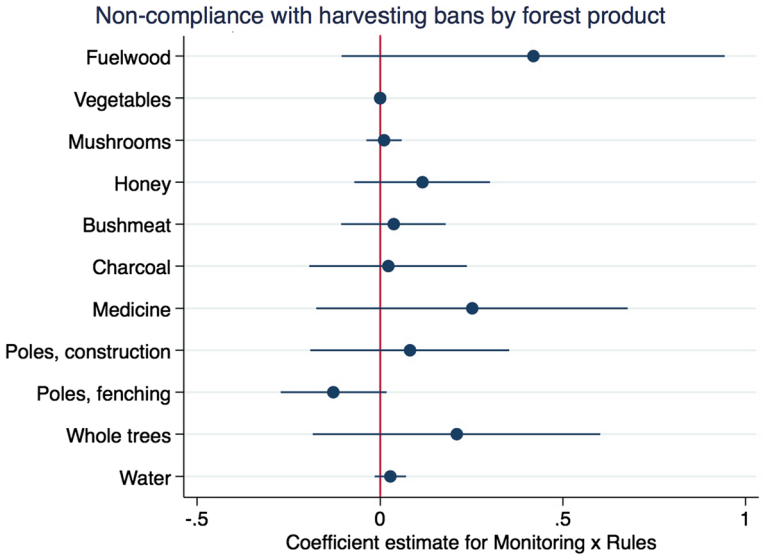

Fig. 3 Effect of rules reminders on non-compliance with harvesting bans for different products. Note: This figure shows the effect of the SMS reminder treatment on non-compliance with harvesting bans for specific forest products. For each of those forest products, we estimate Eq. 1 and display the coefficient estimate for the treatment indicator "Monitoring × Rules". The dependent variable is a standardized dummy equalling 1 if a household collects the forest product in the community forest even though the collection of this product is completely banned

village breaks a rule about forest use, how likely is it that they will receive a penalty?". Column 3 of Table 4 shows that the SMS reminders significantly raise the perceived probability of sanctions for rules violations by 0.4 standard deviations. The effect is statistically significant even when the p value has been adjusted to account for multiple hypotheses testing.

Actual sanctions did not increase significantly as a result of the SMS reminder treatment. This is shown in Column 4 of Table 4, where the outcome variable is an index taking higher values if a household received official sanctions by the forest management organization or was scolded by neighbours.

6.4 Effects on Compliance and Forest Use

The SMS reminders did not raise compliance with forest use rules. If anything, the incidence of rule-breaking in villages receiving the SMS reminder treatment is higher than in the comparison group. Column 1 of Table 5 shows a small, but statistically significant increase in non-compliance by 0.07 standard deviations. The use of the second non-compliance index, which focuses on behaviours targeted by the SMS reminders, allows similar conclusions. The regression in Column 2 shows a statistically insignificant increase in non-compliance by 0.08 standard deviations.

The supposed increase in non-compliance appears to be an artifact of index construction. In Table 5 we investigate which forest use rules are more likely to be violated as a result of the treatment. To be precise, we estimate Eq. 1 separately for each of the individual components of the non-compliance index. Figure 3 displays the estimated effect

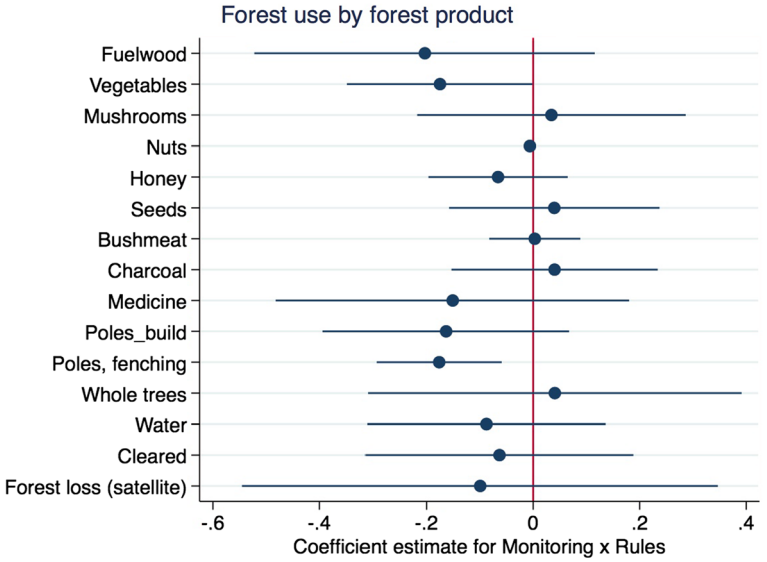

Fig. 4 Forest use by product. Note: This figure shows the effect of the SMS reminder treatment on standardized forest use for specific forest products. For each of those forest products, we estimate Eq. 1 and display the coefficient estimate for the treatment indicator "Monitoring × Rules"

Table 6 Village level forest use measures

	(1) Forest use	(2) Cut trees	(3) Animals	(4) Kilns	(5) Cut branches	(6) Forest loss 1	(7) Forest loss 2
Monitoring	0.095	0.947**	− 0.018	− 0.024	0.047	− 0.002	0.016
	(0.101)	(0.449)	(0.015)	(0.038)	(0.043)	(0.006)	(0.028)
Monitoring*Rules	− 0.078	− 2.037**	− 0.119	− 0.003	0.032	− 0.003	− 0.013
	(0.229)	(0.907)	(0.104)	(0.045)	(0.052)	(0.019)	(0.058)
Control mean	0.012	1.66	0.045	0.041	0.034	0.013	0.032
$\beta_1 + \beta_2$	0.017	− 1.090	− 0.137	− 0.027	0.079	− 0.005	0.003
$\beta_1 + \beta_2$ p value	0.939	0.215	0.204	0.327	0.230	0.785	0.960
Lagged dependent variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	110	102	102	102	102	97	98

Standard errors (clustered at the village level) in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

of the SMS reminders (*Monitoring* × *Rules*) on non-compliance with harvesting bans for the respective forest products. None of the coefficient estimates is statistically significant, indicating that the treatment did not raise non-compliance with forest use rules for any individual forest product.

Compliance with forest use rules is conceptually distinct from forest use. If households still break forest use rules, but do so on a smaller scale, forest use might decrease in the absence of full compliance with forest use rules.

Results do not provide strong support for a reduction in forest use. Column 3 of Table 5 shows the treatment's effect on the household forest use index. The coefficients estimate is negative, but not statistically significant and modest in size at 0.06 standard deviations. The SMS reminders do not lead to a significant reduction of most dimensions of self-reported forest use. Figure 4 shows the effect of the treatment on standardized forest use for all individual components of the household forest use index. We estimate Eq. 1 and display the coefficient estimate for the treatment indicator *Monitoring* × *Rules* in Fig. 4. Only the harvesting of poles for fencing declined significantly (at the 1% level) as a result of the SMS reminders and this effect is statistically significant even after *p* values have been adjusted for testing 15 hypotheses related to forest use. Most of the other coefficient estimates point towards an insignificant reduction in self-reported forest use. Satellite data, which are not subject to misreporting, confirm these insights. The coefficient estimate at the bottom of the table shows that SMS reminders did not lead to a significant reduction in household forest loss based on satellite data.

In sum, the results suggest that SMS reminders did not significantly reduce forest use and violations of forest use rules.

6.5 Forest Use at the Village Level

Results at the village level provide no consistent evidence that the SMS reminder treatment decreased forest use, although forest use is lower in treatment communities relative to comparison communities by some metrics.

There is no evidence that SMS reminders had a statistically significant effect on overall forest use at the village level. Column 1 of Table 6 gives estimates for the impact of the treatment on the village level forest use index. Although the coefficient on Monitoring*Rules is negative and its size is consistent with that observed at the household level, it is not statistically significant at conventional levels.[14]

The remaining columns in Table 6 give results for the individual components of the forest use index, some of which suggest statistically significantly lower forest use in SMS reminder treatment communities relative to community monitoring communities. Note that not all components are available for all communities, as no on-the-ground assessments could be done at endline in 8 villages and cloud cover obscured satellite imagery for several other villages. On-the-ground assessments recorded significantly fewer cut trees along transects in villages subject to the SMS reminder treatment. The coefficient size implies that in these communities, there were two fewer cut trees per 100 m of transect than in communities where households did not receive SMS reminders. However, this result is not statistically significant when correcting the *p* value for multiple comparisons. Results for the other index components are not statistically significant, although the sign on the coefficient of interest is generally negative.

[14] Given that we test the effect of the SMS reminder treatment on forest use both at the household and village level, we conducted multiple comparison adjustments. The effect of the SMS reminder treatment on forest use is not significant when subjecting the household and village level *p* values to multiple comparison adjustments.

6.6 Sensitivity: Results Without Control Variables

This section shows that results broadly follow through if control variables are omitted. Although not pre-specified, omitting the control variables has the advantage of allowing more households from SMS reminder treatment villages into the sample. The number of households that was administered the household survey in these villages was doubled at endline, but these households are dropped from the pre-specified analysis as no baseline control variables were collected for them.

Appendix section C therefore presents results from specification 1 omitting all control variables and shows that the results are similar to those presented in the main text, although omitting control variables does make estimates noisier. This paper shows weak evidence that SMS reminders increase knowledge of forest use rules. This appears to be driven by objective rather than self-reported knowledge in regressions without control variables. Respondents in SMS reminder villages report a 0.2 standard deviations higher perceived likelihood of sanctions upon breaking forest use rules, but this result is no longer statistically significant. When omitting control variables, there is some evidence that the SMS reminder treatment effectuated a decrease in forest use as measured by the household level forest use index, although this is only significant at the 10% level and at 0.1 standard deviations, the observed effect is relatively small.

6.7 Alternative Specification Looking at SMS Recipients

We expect all of the hypothesised effects to be stronger for households that consented to receive SMS reminders. To test this, we estimate a second model which adds $\beta_3 \text{Monitoring}_j * \text{Rules}_j * \text{SMS recipient}_{ij}$ to estimating Eq. 1.[15] $\text{SMS recipient}_{ij}$ is a dummy variable equaling one if a household consented to receive SMS reminders and β_3 captures the effect of the treatment on SMS recipients compared to other households in the SMS reminder treatment villages. SMS recipients were not varied experimentally within treatment villages: households could chose whether to opt in or out of the treatment. Therefore, the results from this regression cannot be interpreted as causal evidence.

Results are captured in Appendix section D. The results highlight that SMS recipients are more willing and likely to sanction others and they perceive scrutiny by others as more likely, both in comparison to other inhabitants of SMS reminder treatment villages and in comparison to pure control villages (Tables D.8 and D.9). The effects are statistically significant and large (around 0.5 standard deviations). However, this does not translate into higher compliance with forest use rules or lower forest use amongst SMS recipients (Table D.10).

[15] This specification was not pre-specified but was included due to the low take-up of SMS reminders in treatment communities.

7 Discussion and Conclusion

This paper investigates the effect of a behavioural intervention designed to improve compliance with rules among users of a common pool resource in Uganda. The intervention employed SMS reminders to improve users' knowledge of the rules, and increase users' attentiveness to them.

We find some evidence that SMS reminders increased knowledge of forest use rules, although this only applies to self-reported knowledge of rules, not to actual knowledge of rules. Attentiveness to forest use rules—as measured by the frequency with which individuals discuss these—was also unaffected. SMS reminders increased the *perceived* likelihood of receiving a sanction upon breaking the rules, but there is no convincing evidence that forest users scrutinized or sanctioned each other more in actuality. SMS reminders furthermore did not induce full compliance with forest use rules, and we find no definitive evidence that this treatment decreased forest use, although forest use did decrease in the treatment communities relative to the comparison communities by some metrics.

The results presented have several implications for the literature on behavioural interventions, specifically in environmental science and for interventions using SMS reminders. The paper speaks to the debate on whether SMS reminders are effective because they increase knowledge (Blanco and Vargas 2014; Fafchamps and Minten 2012) or because they increase attentiveness (Karlan et al. 2016; Larochelle et al. 2019). We find no evidence that SMS reminders increased our pre-registered proxy for attentiveness, but we find evidence that they increased knowledge of forest use rules. However, better knowledge of rules did not translate into reduced forest use. This may suggest that better knowledge alone does not change forest use.

Furthermore, this paper suggests that SMS reminders can affect the perceived likelihood of sanctioning. This echoes other studies that suggest that the fear of sanctions can be manipulated in absence of an increase in actual enforcement efforts (Bateson et al. 2013; Nettle et al. 2013; Burnham and Hare 2007). This effect does not follow from a standard neo-classical model and is purely behavioural.

An increased fear of sanctions did not achieve a significant increase in compliance with forest use rules or a decrease in forest use. Thus, future research might usefully explore how a behavioural intervention akin to the one in the present study might be strengthened. Several suggestions come to mind. First, it is possible that SMS reminders have to reach a critical mass of conditional cooperators in order to be effective. Rustagi et al. (2010) suggest that if conditional cooperators come to believe that others will contribute to the public good—in this case forest conservation—they will too. This could imply an SMS campaign reaching a greater number of village inhabitants, or one that is more targeted at individuals likely to be conditional cooperators – for example because they contribute to other village-level public goods. The findings provide some support for this idea, as the effect of the SMS campaign on scrutiny and sanctioning outcomes for recipients who self-selected into the treatment is considerably stronger than the effects on the average household in SMS reminder villages. Second, even if households judge the probability of sanctions to be higher, they may be incapable of complying with forest use rules or decreasing forest use: households may rely on forest-derived income to survive or they may lack alternatives for forest products. This suggests combining a behavioural intervention with an intervention that compensates forest-adjacent people for decreasing their forest use, such as Payment for Environmental Services, or with an intervention decreasing the costs of alternative fuel and building materials. Third, behavioural science suggests that intentions, in this case the

intention to decrease forest use, do not always directly translate into behaviour (Sniehotta et al. 2007). The behavioural intervention in this study could thus be paired with an intervention that helps individuals plan specific ways to reduce forest use.

Even though behavioural interventions have successfully induced pro-environmental behaviour, the SMS reminders sent out as part of this study did not reduce forest use. There are several potential explanations which warrant further research. First, SMS reminders have not been trialed in environmental economics, even though they have successfully increased desired behaviour in the areas of health, loan repayment or agricultural practices. Second, most successful behavioural nudges in environmental economics focus on descriptive norms (what others do) or combinations of descriptive and injunctive (what one should do) norms. This study, on the other hand, studies injunctive norms in isolation. A small number of studies in this area yields mixed results: Cialdini et al. (2006), Bhanot (2020), de Groot et al. (2013) show that appealing to injunctive norms can reduce the removal of petrified wood (Cialdini et al. 2006), energy use (Bhanot 2020) and plastics bag use (de Groot et al. 2013). Schultz et al. (2008) on the other hand, finds that injunctive norms in isolation did not reduce the use of towels in hotel rooms. Third, behavioural nudges have been employed in electricity, water use and recycling, but not in a field setting relating to forestry or common pool resources. Unique dynamics around public goods or common pool resources could plausibly reduce the effectiveness of behavioural interventions. Finally, behavioural nudges in environmental economics [as well as in other areas such as tax compliance (Anderson 2017; Fonseca and Grimshaw 2017; John and Blume 2018; Sanders et al. 2008; Iyer et al. 2010)] have been shown to be very context-specific (Schubert 2017). Future research should investigate whether SMS reminders or similar nudges could offer a low-cost opportunity to conserve private or communal forests in different countries and contexts.

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